

( 125998)Part A1 Creationism 2nd EDIT

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<uf920><BF>Q. We discuss science as if we understand precisely what it is. The issue doesn't seem that simple. Those who disbelieve the theory of evolution, for example, have proposed in its stead creationism. Is creationism a science? <LF>

<BF>Dr. Kuhn. <LF> I don't have a nice set of necessary and sufficient conditions for being a science. But with creationism, there isn't a research basis. The activities and objectives involved are not those of solving a succession of internally generated puzzles. In those terms, I don't think creationism is a science.

I would say there's a group here that makes knowledge claims. They don't make them on the basis of the same sort of enterprise scientists use. That would not mean that scientists are right and creationists are wrong, but it would make it absurd to suggest that the two enterprises are the same.

<BF>Q. Surely science is defined by its method -- the scientific method -- not by its content. <LF>

<BF>Dr. Kuhn. <LF> I don't believe there is something specific as scientific method that is not pretty much what common sense calls for in almost any area.

<BF>Dr. Lederberg. <LF> I would call creationism a theory of knowledge, because there is a pure form in which its propositions can be stated that is undefeatable. That form is that at some point in time, perhaps just one microsecond ago, the universe was created and that all the evidence we have access to and all the evidence we will ever have access to may convince us that there was a long evolutionary tradition leading to it.

I view that as a logically undefeatable proposition. For having said it, what do you do next? You might posit that for a variety of reasons, such as predicting what you'll discover the next time you put a spade into the ground, you wish to pursue your investigation of the evidence created one microsecond ago to see what other kind of consistent picture emerges. You then end up with precisely the same enterprise as the sciences pursue.

<BF>Dr. Kuhn. <LF> In practice, however, creationists did not develop their viewpoint in that way and have not been tempted to do the sorts of things with their viewpoint that scientists have done.

<BF>Q. What about the notion that scientific method is largely common sense? <LF>

<BF>Dr. Lederberg. <LF> To the extent that very little by way of

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scientific demonstration follows formal procedures, it is pretty much common sense. But it's commonsensical once you look at it. Scientists use methods we should recognize in our daily lives, but that we may not push to the ultimate. For example, how often do we go to our adversaries' positions and ask whether they are logically defeasible? That method is common to scientific inquiry.

<BF>Dr. Kuhn.<LF> You're right. Science is more systematic. But its logical structure is very much the logical structure of common sense. And it's in that sense that you're not going to be able to distinguish a science from a non-science on the basis of the method used.

<BF>Dr. Lederberg.<LF> There's a relentlessness, though, in science rarely found in everyday affairs. Indeed if we did find it, we'd call the person who operated in this way compulsive.

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<BF>Q. To what extent do you think that science today has been politicized? There was, for example, the trial in Salt Lake City where scientists disputed whether radioactivity from atomic tests in the 1950's caused subsequent disease among Utah citizens. It seems that science asks people to believe in the reliability of results. Yet here was conflict. <LF>

<BF>Dr. Lederberg. <LF> Why do you call that politicization?

<BF>Q. Because radioactivity is a politically charged issue. <LF>

<BF>Dr. Kuhn. <LF> It certainly is something discovered and studied by scientists that has gotten involved deeply with questions that are political. But I would like to talk about this as something that involves scientists with politics, not as the politicization of science.

There are more areas today than in the past in which the products of research are of vast social consequence, that have political overtones. And this makes differences. Among them, it is creating a new self-consciousness on the part of scientists. Part of the problem is that the public misapprehends the extent to which expertise in the scientific aspects of, say, radioactivity enables scientists to produce answers on all social or political questions raised by the use of radioactivity.

<BF>Dr. Lederberg. <LF> In Utah, at least, a large part of the controversy concerns the amount of radioactivity released and the nature of human exposure. The answers have a lot to do with the records that were kept and the credibility of the institutions involved. Of course, people's institutional affiliations are going to have a lot to do with what they say they believe to be the authenticity and veracity of records of past events.

<BF>Q. Isn't that politics? <LF>

<BF>Dr. Lederberg. <LF> Exactly. But I don't know if that's politicizing science. There is a political issue in which people are wearing a variety of hats, and the ones called in to testify are selected because they are willing to express polarized views one way or the other.

<BF>Q. There is an interesting conflict here, because politicians, lawmakers, are required to pass legislation. <TH> <TH> <LF>

<BF>Dr. Lederberg. <LF> Yes, they want one-armed bandits.

<BF>Q. <TH> <TH> and they turn to scientists for a substantive basis for their rulemaking. Often, however, science isn't developed enough, or doesn't have the experimental evidence to provide such a basis. How should scientists deal

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with legislators' demands?<LF>

<CBF>Dr. Lederberg.<LF> The scientists' job is to tell them what the health risks are; the value judgments belong to a much larger sphere. That's a somewhat naive theory of separation, but I think it's something we ought to aspire to.

<CBF>Dr. Kuhn.<LF> I don't disagree with anything you've said, but it doesn't answer what, for me, is the crux of the question.

There are policy decisions that must be made to which scientific findings are relevant, but for which the findings are not precise enough or the theories are not developed enough to come out with anything but the most broad-gauge ranges. There are political pressures, then, for scientists to give relatively precise answers that will be of some use to policymakers when it's not always possible to give precise answers. As a result, one gets terribly, terribly hung up. Then, too, how the devil one manages to get appropriate sorts of scientific input \_ which seems absolutely essential \_ from a science that isn't advanced enough to give the sort of answers that are wanted, I don't know.

( 125996) Part C1 Sci. & Change 2nd EDIT

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<uf920><BF>Q. A central theme of Dr. Kuhn's book is that to a great extent, non-rational factors cause scientists to accept new ideas. Dr. Lederberg, do non-rational factors play a role in genetics?<LF>

<BF>Dr. Lederberg. <LF>There's a consensus that I played an important part. For example, I was quite startled at age 21 to have made a surprising discovery that involved merging bacteriology and genetics. That was contrary to the wisdom of the time, which held that bacteria could not be crossed since they had no genetics.

I've been puzzling about that ever since, because I felt the discovery should have been made 20 years before I was born. One can hardly give a rational explanation for the fact that it had not even been looked for.

<BF>Q. What popped the notion of crossing bacteria into your head?<LF>

<BF>Dr. Lederberg. <LF> Well, the work of other scientists had made it important to discover whether bacteria had genes or a genetics that was consistent with the mainstream of genetics research in mice and other animals. Before that time, the issue may not have been important.

<BF>Q. Did you meet with disbelief?<LF>

<BF>Dr. Lederberg. <LF> I might have. But coincidentally, (in 1946) there was the first postwar symposium on genetics. I don't think anyone important in the field was left out. Person after person had gotten up and said how exciting the work was, but isn't it too bad that bacteria don't have a sexual process. It was an irresistible setting in which to say, 'But they do.' And I did.

There followed three or four hours of intense, critical discussion during which all the correct questions could be put and the bulk of scientists could convince themselves that my experiments had no loopholes. That rarely happens.

<BF>Q. Until the time of your discovery, however, was there an unspoken dictum that bacteria do not have a genetics?<LF>

<BF>Dr. Lederberg. <LF> Oh yes. Certainly among bacteriologists. It's embodied in the class name. In the scale of nature, it was one of the distinctions by which bacteria were differentiated from more complex organisms. It had been a philosophy of the distribution of life, deeply ingrained.

In fact, it had become almost a canon of faith that if you were a pure bacteriologist working with uncontaminated

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nated cultures, they didn't do anything interesting from a genetics point of view. Even when there were a few undeniable observations that bacteria vary sometimes -- well, that was believed to be the result of contamination.

<BF>Dr. Kuhn.<LF> In my terms, I find that extremely plausible. I mean, a bacteriologist was taught to stop bacteria from changing. Almost by definition, that's what was meant by bacterial cultures being pure. So that at an early stage of the game, built into the scientist's notion of a pure bacterial culture, was the notion that it doesn't change. And the first thing you did when you saw changes was to assume impurity. That type of assumption made it extremely hard to discover that there are, indeed, genetically borne changes in bacteria.

So, you see, it isn't just stubbornness that leads people to hold on to an outmoded belief. This is something built into scientific language and technique -- and that's what I'm talking about. In the case of the bacteria, you'd have to change your ideas of the appropriate techniques for purification in order to accept a discovery of the sort Dr. Lederberg made.

<BF>Dr. Lederberg.<LF> I want to more efficiently pursue the use of such insights into scientific change to promote more rapid scientific progress. I wonder what Dr. Kuhn would advise that might rationalize the overall process?

<BF>Dr. Kuhn.<LF> I'm not sure as to how much rationalization need be done. Clearly there are times when you're going to say somebody's going overboard on something; there are extremes one wants to avoid.

But look, you're saying that one of the reasons your field didn't change earlier was because nothing had rubbed the noses of bacteriologists and geneticists in each other's work. My guess is that if somebody had tried to bring those groups together earlier on, without something substantive on which they could focus and evidence that it was a good area to focus on, nothing would have happened.

I am inclined to say, then, that evolutionary patterns and internal developments are what most fruitfully bring two groups of this sort together, and I'm dubious as to whether one can speed and rationalize the process.

<BF>G. Is scientific change largely accidental then?<LF>

<BF>Dr. Lederberg.<LF> Institutional forms have consequences, and something can be done about those forms.

<BF>Dr. Kuhn.<LF> I don't mean to say that institutional forms don't make a difference. But in most cases, they've made a difference because of particular things going on in the

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sciences, which were then permitted to come together or facilitated in doing so. If the fields had been in different states or if other fields were put together, again it's likely nothing would have happened.

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(CBF) Dr. Lederberg. (LF) Permissions for disciplines to meet aren't that easy to come by. There are many institutional settings where scientists would not be allowed to change the character and direction of their investigations or to enter fields in which they did not have credentials. I would argue that creating environments where these things are permissible, even if you can't force two nuclei to fuse, is an important issue of science management. (CBF)

<Cuf920><CBF>Q. What new proposition or discovery would be the most earthshaking for science today? The discovery of life forms in outer space?<LF>

<CBF>Dr. Kuhn.<LF> It depends on what the life turned out to be. There's a story in contemporary philosophy that illustrates what I mean. A space ship from Earth goes to a place called Twin Earth, which is very much like Earth. There's even this liquid that lies around in lakes. Twin Earth's inhabitants drink it and it evaporates and rains down again. In fact, on Twin Earth it's called water. But when the chemist from Earth gets out and analyzes the liquid it's not <Cuf908>H#2#O, it's XYZ.

The message that is supposed to have been sent back to Earth at this point says 'On Twin Earth water is XYZ not <Cuf908>H#2#O.' But that's absurd. The wire should have read 'Back to the drawing boards -- our chemistry is all wrong. It doesn't have a ruling for something that behaves like water and isn't <Cuf908>H#2#O.' That kind of discovery has got to be revolutionary, because it's incompatible with the fundamentals of existing science.

<CBF>Dr. Lederberg.<LF> I would think that just finding a Twin Earth, in the sense of a planet with an evolutionary pattern similar to ours, would be shattering. It would imply determinism of a series of events to which we now impute a highly random character. The preferred hypothesis would be that the Russians got there last year and planted a colony, not that there had been parallel evolution congruent to our own.

<CBF>Q. Is there intellectual resistance to such a search?<LF>

<CBF>Dr. Lederberg.<LF> Yes of course, and it's probably unreasonable; that is to say the proposition has not been tested to the point where you ought to give it away as insoluble.

<CBF>Dr. Kuhn.<LF> I think the real question here is one of risk-benefit analysis. Whether one should pursue this search depends partly on how much it would cost and partly on how rewarding it would be to know the answers. We are going to have to husband resources and ask questions of that sort of scientific research.

<CBF>Q. But this husbanding comes when scientists hold to popular theories in virtually every field.<LF>

<CBF>Dr. Kuhn.<LF> Are you suggesting that because of a resource shortage quirky people will be prevented from pursuing less popular theories?



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<BF>G. Yes.<LF>

<BF>Dr. Kuhn.<LF> That happens, particularly where big machinery is involved. But what is the alternative? We need more money and machines so that people with less popular views can test them. It would help the sciences if that happened. But is it worth the financial costs?